

BEYOND THE GANZFELD DEBATE

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The Editors of this *Journal* sent me prepublication copies of Ray Hyman and Charles Honorton's "Joint Communiqué" on the ganzfeld controversy (Hyman & Honorton, 1986) and Robert Rosenthal's extensive and valuable commentary on that debate (Rosenthal, 1986), and they kindly invited me to submit my own reactions, comments, or suggestions to be published along with those papers in the present issue of the *Journal*. After I had read the papers, I immediately and willingly agreed to write up my comments and, although there were only a few days left because of the publishing deadline, I thought it would be easy to comply with the editors' request. But now that I am actually sitting down to write up my reflections, I realize that there is so much to comment on in these papers (as well as in the earlier ones), and so little time left to do so, that I find myself forced to concentrate on just a few of the particular methodological, terminological, and scientific-political aspects, side-issues, and consequences raised by the ganzfeld controversy. Therefore, no comments will be offered here on the numerous technical, statistical, or meta-analytic issues raised in Hyman's (1983, 1985a) and Honorton's (1983, 1985) respective contributions to the debate; it would be irresponsible of me and a mockery of the enormous amount of time and work that both Honorton and Hyman must have spent on their studies if I presented such comments without having done a sufficiently careful and detailed (hence time-consuming) reanalysis. Nor will I comment here on Rosenthal's interesting thoughts on "the nature of replication" (I hope to do that in an extensive paper, now in preparation, on "The Repeatability of Parapsychological Experiments: Problems, Conceptualizations, Consequences"). Instead, I will present some comments on the specific methodological recommendations given by Hyman and Honorton in their "Joint Communiqué," make some casual remarks on a terminological problem mentioned by the authors, and offer some reflections on the communiqué's scientific-political implications for rational communication and collaboration between (what some have described as) the parapsychological and the skeptical camps.

METHODOLOGICAL RECOMMENDATIONS

Hyman and Honorton (1986) make several useful recommendations about how future ganzfeld (and maybe other) experiments should be conducted and reported. I find myself in full agreement with their specific remarks, and there is hardly anything substantial I have to add at this point. However, based on my own experiences as a subject in a ganzfeld experiment, I would like to suggest at least one addition to the requirements *a* through *e* mentioned in the "Judging and Feedback" section of Hyman and Honorton's paper: In my view, it is important to insist that the subject-receiver is left alone during the judging procedure; that is, after the receiver's experimenter has presented the subject with the judging pool and has handed to him the instructions for judging, he should leave the subject alone and return no sooner than the subject signals the completion of judging. It must be made certain that it is the subject, and the subject alone, who does that judging. This cannot be guaranteed if the subject's experimenter stays with the subject during the judging procedure. Even if the former does not know the identity of the actual target (which, of course, is absolutely essential in any case), and if he does not speak a word during the judging procedure, his nonverbal behavior still may have some influence on the outcome of the subject's judging. This rule, which to me seems self-evident, was not observed in the ganzfeld trials in which I participated as a subject or as an external observer.

Also, I would like to add one further, more general, recommendation. There is a comparatively small number of technical, refereed periodicals in the field of parapsychology. These include the *Journal of Parapsychology*, the Journals of both the American and British Societies for Psychical Research, the *European Journal of Parapsychology*, and the *Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie*. In my opinion, it should not be too difficult for the respective editors to join forces and set up, along the lines suggested by Hyman and Honorton, a canon of obligatory criteria of acceptability, of empirical reports at least.

TERMINOLOGY

Not surprisingly, I was pleased to see that, in footnote 3 of their paper, Hyman and Honorton, using the term *psi* as their example, are drawing the same distinction between *identifying* and *explaining* a

communications anomaly that others (Hövelmann, 1983; Hövelmann & Krippner, in press; Palmer, 1983) have drawn before¹ and that, in the meantime, has found its way into the Parapsychological Association's (1985) *Report I: Terms and Methods in Parapsychological Research*. Also, I do agree with the authors' views on the possible relevance of the maintenance of that distinction for, among other things, the improvement of future communication between parapsychologists and other scientists. On careful reading of their paper we find, however, that their own usage of the term *psi* is not always consistent with the insight they formulate in footnote 3. Although in that footnote they state that "consistent with the original usage [as suggested by Thouless and Wiesner], the term *psi* in this paper simply denotes a communications anomaly," we come across the following sentence on p. 353 of their paper: "If *psi* is responsible for the outcomes obtained in this data base, then the ganzfeld experiment should continue to produce successful outcomes when the various problems that Hyman pointed out are eliminated." (My emphasis.) Obviously, the latter formulation does not simply refer to a communications anomaly but rather presupposes a more-or-less well-defined "responsible" factor or an explanatory concept. Even with best intentions, it appears difficult to free oneself from ways of talking one has been accustomed to.

POLITICAL IMPLICATIONS

In footnote 2 of their "Joint Communiqué," Hyman and Honorton (1986) state that "to the best of our knowledge, this is the first time a parapsychologist and a critic have collaborated on a joint statement of this type" (p. 351). Although, strictly speaking, their "Communiqué" is not the first joint statement by parapsychologists and critics,² it certainly is the first one that has resulted from a detailed, in-depth discussion of a narrowly defined parapsychological problem area. In fact, this "Joint Communiqué" is quite the sort of

¹ Also, see the useful discussion provided by Zingrone and Alvarado (in press).

² There is at least one earlier consensus statement by "parapsychologists" and "critics" that resulted from an informal meeting that took place in Marburg, West Germany, on November 7, 1982. All participants—a Dutch critic (P. H. Hoebens), a German critic (I. Oepen), and three German parapsychologists (E. Bauer, G. H. Hövelmann, W. von Lucadou)—were able to agree on a catalogue of nine basic statements about parapsychological research and its implications and on the relationship between proponents and opponents. Long extracts from that consensus statement, which has become known as the "Marburg Manifesto," were subsequently published by Frazier (1984) in the pages of the *Skeptical Inquirer*.

thing I have long been hoping for, and I think we should have nothing but admiration for Hyman and Honorton's highly successful endeavor. They have managed to emphasize what they (justly) believe are the "broader and more important propositions" (p. 352) concerning which they find themselves in agreement (and on which I agree as well) without having to cloak their disagreement on a number of technical and other details.

Hyman and Honorton's "Joint Communiqué" does have a variety of important consequences and implications for future communication between parapsychologists and their critics (if that distinction makes any sense at all). Previous controversies around parapsychological research and its legitimacy as a scientific endeavor have lost sight of, and extremists on both sides have dissimulated, the fact that parapsychologists and their critics have many common objectives. The Hyman and Honorton paper is a most welcome reminder of these common goals, which rational minorities in (or maybe rather between) both camps have been tirelessly emphasizing for a while. It is high time now that both parapsychologists and their critics become more aware of these common objectives. It is high time that parapsychologists and critics join forces on a larger scale and look for ways toward closer and more fruitful collaboration. And, in my opinion, it is high time that all parties concerned work hard on eliminating the deplorable parapsychologist-critic dichotomy as soon as possible and as thoroughly as possible. As readers of my earlier writings on the relationship between parapsychologists (so-called) and critics (so-called) may recall, I have always expressed dissatisfaction with (and I have questioned the adequacy of) that dichotomy, because I have never been able to understand why skepticism should not form a proper part of parapsychology, and why those whom John Palmer has called "conventional theorists" (Palmer, 1986) should not wish (or be allowed) to do their own "parapsychological" research and empirical tests of their own or others' hypotheses. After all, the only thing that matters is the quality of the scientific work and the quality of arguments. So what is needed is close collaboration between all the parties involved. Useful and practicable suggestions for such future collaboration are already available (for instance, see Truzzi, 1985).

In a recent paper, Hyman (1985b) wrote:

Unfortunately, as any reading of the history of psychical research quickly reveals, the psychical researchers are correct in their appraisal of their critics. Too often, the major critics have attacked straw men and

have not dealt with the actual claims and evidence put forth by the more serious researchers. (p. 6)

and

Since its inception as an institutionalized undertaking, psychical research has suffered from the lack of relevant, informed, and constructive criticism. (p. 87)

and

Indeed, the level of the debate during the past 130 years has been an embarrassment to anyone who would like to believe that scholars and scientists adhere to the standards of rationality and fair play.³ (p. 89)

This appraisal of the quality of skeptical reactions to parapsychological research seems to be supported by a recent survey of the skeptical literature (Hövelmann with Truzzi & Hoebens, 1985). However, as Hyman (1985b, 1986) also has pointed out, the situation has been improving on both the parapsychological and the skeptical side of the fence. This fact is convincingly attested by Hyman and Honorton's joint paper.

Admittedly, expressing delight is not exactly an adequate scientific response to the outcome of a scientific controversy. Nevertheless, I do not hesitate to state here that I am delighted with Hyman and Honorton's "Joint Communiqué."

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³ The same passages are to be found in Hyman (1986) on pp. 824, 847, and 848, respectively.

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